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M. RICORD'S LETTERS UPON SYPHILIS.

Addressed to the Editor of L'Union Medicale—Translated from the French by D. D. SLADE, M.D. Boston, and communicated for the Boston Medical and Surgical Journal.

ELEVENTH LETTER.

MY DEAR FRIEND,—We must now determine the source, where the specific cause, the morbid poison which produces syphilis, is to be found. This poison, we can at the present day call by its name, the *syphilitic virus.*

Well! this virus—I must needs recall the circumstance, inasmuch as endeavors have been made to cause it to be forgotten— was formally contested and denied, when I undertook my first researches in syphilopathy. This was the time when numerous physicians did not dare to give it this name without fear of compromising themselves. It was the time when the learned Jourdan, in an access of singular anger, cried out—" call it as you will, but do not give it the name of virus."

The source of this virus, I have obtained at the point of the lancet, upon which, however, I have not had the pretension of placing all science, as my honorable colleague, M. Cazenave, wittily accuses me.

It is in studying comparatively all the accidents reputed syphilitic, that I have succeeded in demonstrating that one alone of these accidents would furnish regularly the purulent matter; capable, in placing it under conditions which we shall determine, of producing, in virtue of a special irritation, an ulcerating inflammation identical to that which has been the source of it, and of reproducing in its turn the same special secretion, the same morbid poison, and this without limit.

The syphilitic lesion, source and origin of the secretion, placed in favorable conditions, produces fatally the phenomena which we have just indicated, and which is the primitive accident to which has been given, and which has preserved the name of *chancre.*

Every time, as I have already had occasion to remark, that we were able to see the surfaces from which we took the morbid secretion, which should serve for experimentation, it is only when there existed a chancre, that positive results could be obtained, and that we were able to reproduce the chancre.

Must I again say that my excellent colleagues, M.M. Puche and Cullerier, at Paris; M. Baumès and Diday at Lyons; M. Renault at Toulon, Serre at Montpelier, M. Thiery at Brussels, M. Lafont Gouzy

at Toulouse, &c., have arrived, in their very numerous experiments, absolutely at the same results as myself.

Every time that the chancre could be produced with a secretion which had not been taken immediately from a primary ulcer, the secretion was furnished by surfaces which could not be inspected. The small number of cases, exceptional in appearance, in which the chancre could be reproduced with a purulent matter taken from a non-ulcerated surface, find their rational and absolute explanation in facts analogous to those of which I have recounted the history. How can it be concluded that the surfaces which cannot be inspected are not the seat of chancre, inasmuch as they furnish absolutely the same secretion as the chancre? Ah! if it was proved that the primary ulcer, fatal source of the syphilitic virus, could not be seated, excepting upon external surfaces which are always visible; that the depths of the urethra, and the cavity of the neck of the uterus, could not be the seat of these concealed ulcerations —if this was proved, all would be said; but does there exist one sole writer upon syphilis who denies the existence of the primitive ulcer upon all these regions, who does not know and who does not believe that all syphilitic ulcerations are not always visible? How, then, can we deny the possibility of the existence of deep and concealed chancre, when it in itself furnishes the most undeniable proof, that is the secretion?

It has been said that inoculation cannot serve any purpose in proving the existence of the specific cause of syphilis; that it was preferable to confine ourselves to the ordinary results of contagion to arrive at this proof; for with any pus whatever we can produce what I pretend to produce only with the pus of the chancre, while by the mysterious ways of common contagion phenomena are observed, which inoculation does not produce.

It is at least strange that these same arguments are equally employed, both by the maintainers of the syphilitic virus, and by those who deny its existence. In fact, what do these physiologists say? That with any pus whatever, that with a cause no matter what, the same result was arrived at—that is to say, the production of every variety of venereal disease. And upon what do they rely to sustain this doctrine? Upon motives which could then appear reasonable; upon all the uncertainties which ordinarily exist under the circumstances in which the venereal diseases are contracted; for the want of examination of women; upon the great number of the accidents determined by the same woman upon several men, while this same woman could leave other men entirely indemnified from evil consequences; finally, upon all the fables that we have already signalized and combated, and upon which one is truly astonished, after what the speculum has discovered, to see men of merit as incontestable as M. Cazenave wish still to ground superannuated doctrines.

But I am profoundly astonished that the partizans of the syphilitic virus, those who recognize in syphilis a specific cause, and in its virus a specificity of action, sustain, that with any pus whatever effects can be produced analogous to those of the inoculation virulent *par excellence*. Do the partizans of these doctrines think that we could produce vac-

cinia or the variola by any kind of pus? If it was given to them to experiment upon purulent matters the source and origin of which they were ignorant, what would be their criterion for determining the nature of them, if it was not the effects produced? Is it not in this way that I arrived at distinguishing the syphilitic pus?

But to this objection *of any pus whatever* as proof of the inutility of inoculation, I have another thing to answer.

I have inoculated the same patient, and that a hundred times, with the pus of chancre, of balano-posthitis, with the muco-pus of urethral blennorrhagia, with the muco-pus of blennorrhagic ophthalmia, with the pus furnished by the phlegmonous inflammations of other regions; and while that of the chancre inevitably reproduced the chancre, the other kinds of pus remained without action. What do they want more than this proof, and what can they answer to it?

Another objection, however, has been made. They have said, the inoculation does not prove anything as to the nature of the cause, from the effects that it can produce upon an individual already submitted to the infection; in other words, in inoculating the patient with the secretion that he himself furnishes, no conclusion can be arrived at, inasmuch as that if infected every wound can and ought to become syphilitic.

Herein is a strange error, the consequences of which might be very grave; a dangerous prejudice, which I am astonished to see again brought forward in our day with the sanction of observers who make pretensions to exactitude and precision. The facts which I have just recalled peremptorily destroy this objection. I well know that facts relating to leech-bites, for example, have been cited, which have taken on later the character of venereal ulcers. But be assured, my friend, these bites, like every wound in a syphilitic patient, do not become virulent ulcers, unless they are finally infected by contagion. Apply leeches where there has been no contact with inoculable pus, bleed the syphilitic patients as much as you wish, practise any other operation whatsoever, and never, unless there has been virulent contact, will a virulent transformation be possible. Among the numerous observations which I have collected in proof of the truth of this assertion, I will recall the following fact of the Hospital du Midi.

At the period when I had women in my wards, a patient affected with a phagedenic chancre of the vulva, with abundant suppuration, was seized with a pain in the tibio-tarsal articulation. Leeches were applied upon the painful spot. Some days after, the patient complaining at the seat of the bites, it was easy to recognize that some had undergone a veritable transformation, and that they had become veritable chancres. One could believe for a moment in the influence of the general condition of the patient, and some of the students believed in it. As to myself, I had not the least doubt upon the mechanism of this transformation. In the first place, all the bites were not ulcerated—first proof. Secondly, the patient was seized with similar pains in the articulation upon the opposite side; a new application of leeches was made, but this time, in guarding the bites from every infecting contact, none of them underwent the least syphilitic transformation.

I have made an experiment more conclusive still. It has often happened that I had to experiment with the pus of a chancre upon a patient even then under the influence of a constitutional syphilis determined by a preceding contagion. Some comparative inoculations were made, and then again the matter of the chancre alone gave place to positive results. Thus, whatever may be said, it is impossible to compare a syphilitic patient to a bottle full of virus, which it would allow to escape through the smallest opening. The image is poetical, but it is not just.

But in order that these results should be inevitably obtained, reason tells us, in the first place, that the *virulent matter* ought to be taken from a chancre at a certain period—that is to say, at the period of progress, or of specific *statu quo*. It is very easy to conceive this, and I am sure of not fatiguing you in trying to make you understand that if you take the pus for inoculation from the surface of an ulcer which is in process of reparation and of cicatrization, you will have a simple and inoffensive pus, which will give you negative results, and that the same accident interrogated at two different epochs will say to you, yes and no. You will conclude, then, with all observers of good faith, that there is here no contradiction in the results of experimentation, nor uncertainty, and that it is no evasion, no subtilty of doctrine, to explain facts opposed to the principles which I sustain, and similar to those of Bru. When Bru did not succeed in inoculating the pus of chancre, one of two things happened; either he made a false diagnosis and directed his attention to other ulcerations, or he took the pus from chancres *at the period of reparation*. There is no way of escaping from this dilemma; for I repeat it, and I am ready to prove it to the incredulous, if there are any still, *the pus of the chancre is inevitably inoculable*.

You will perhaps find, my friend, that I suffer myself to go too far in the pleasure of writing to you: but it is your fault, you never stop me. Profiting, then, by your good will, I will say that if the *virulent matter* composed of a special morbid poison and of a vehicle, is ordinarily formed of a thin, ichorous, sero-sanious pus charged with organic detritus, it does not always present itself with the same characters; it can offer all the known varieties of pus or of muco-pus. It can be acid or alkaline, contain animalcules or not. These different conditions which appear contradictory, and have also served as an argument to those who deny the existence of a virus, belong only to its vehicle, and change nothing of its nature, which remains always the same. There is but one circumstance important to signalize, and which experiments upon inoculation have verified—viz., that the putrid pus is not virulent, that gangrene destroys the virus—*it kills it*.

In order to act, whatever may be the seat of the chancre from which it has been taken, the virulent matter has no need of being recently secreted and warm. Preserved as vaccine is, it acts equally well. Artificial inoculation proved this, contrary to the opinion of Cullerier, which hitherto was in vogue in science.

Inoculation has proved the truth of the different modes of contagion, more or less contested, so far as necessity of the physiological action and

of the orgasm of the part which furnished the contagion was believed in, and as it was thought that this ought to be yet warm at the time of infecting. The observations of Fallopius and Hunter of chancres contracted in touching the seats of public privies; those of Fabricus of Hilden of accidents taken in sleeping in sheets in which infected persons had already slept, and of so many others, in fine, have thus become incontestable.

You will still permit me to say a word upon the conditions in which the part which one inoculates ought to be. Whatever it may be, skin or mucous surface, no matter what region, it suffices to have *a simple solution of continuity*, without *the aid of any physiological act, in order that the effect should be inevitably produced*: there is nothing here, as in the case of the variola and vaccine, which resists the primitive accident; there is no privilege of idiosyncrasy; the most perfect equality exists in the presence of a point of a lancet charged with virulent matter.

Thus, then, dear friend, the inoculation made with the pus coming from a primary accident, with the pus of a chancre, in the condition which I have just recalled, has always produced identical results, whether experimentation has had for subject the patient who furnished the pus, or whether the pus has been inoculated from an infected to a healthy individual, as some experimenters have done.

It has, however, again been said—it is imprudent, rash and impossible to conclude anything from artificial inoculation; you impose upon nature conditions other than those in which she is placed during the contagion which we can call natural by contradistinction. And condemning this artificial inoculation, they thought they could say of it what is said of physiological experimentation—"Torture demands and pain responds."

Our celebrated physiologist M. Magendie, to whom you addressed your first, and so remarkable *medical letter*, will tell you what he thinks of this indignation of the poets. As for myself, who do not wish to speak with the same authority, I shall say that I do not contest the mysteries of nature, that I know she does many things by processes which she conceals from us. But I maintain, also, that it would be an unworthy weakness to seek to render her still more mysterious, and to thicken the veils which cover her; that it would be shameful to shut our eyes when she wishes to unveil herself.

Let us see, then, if there exists any real difference between the natural and the artificial contagion. I shall tell you what I think of this in my next letter. Yours, &c. RICORD.

INTERESTING STATEMENT OF FACTS RELATIVE TO THE ETHER DISCOVERY.

To the Editor of the Boston Medical and Surgical Journal.

SIR.—A short time since, there appeared a "Supplement of the Boston Daily Evening Transcript," containing the debate in the United States Senate upon the ether question; re-published from the Washington Daily Globe as an advertisement. I regret that your valuable Journal

was made use of to give further currency to this debate. Under ordinary circumstances no objection could be made to a fair discussion of this controversy upon its merits; but the extraordinary circumstances of this debate, and the pernicious nature of a series of false statements that were thus given to the world through the mouths of some members of the Senate, render their re-publication a libellous wrong upon Dr. Jackson, only equalled by the nature of the circumstances under which the debate arose. The imposture was voted down by the tremendous vote of 28 to 17, notwithstanding the one-sided and *ex-parte* representations that were made at a time when no opportunity was given to answer them.

It will be remembered that the ether discovery was in the hands of a select committee of the House of Representatives, who had not yet reported, when the subject was called up in the Senate, just before the adjournment, and while one of the appropriation bills was being hurried through, to which an amendment was proposed to award $100,000 for this discovery. An *ex-parte* statement of facts, purporting to be a report of the majority of the select committee of the House of Representatives, was placed upon each Senator's table just at the lighting of the candles on the memorable evening of the 28th of August last. It is sufficient to say of this monstrous report, that it was surreptitiously published and paid but little respect to the evidence in relation to this discovery, which was very full and complete, before the committee of the House. It was characterized mainly for its perverse special pleading in favor of W. T. G. Morton as a claimant for this discovery. Yet, notwithstanding this plea, the report *admits* that the discovery is truly that of Dr. Charles T. Jackson, of this city, in the following words, viz.:

"In closing his statement of the last and final experiment in 1841–2, in the part of the letter of December 18, 1851, to Baron Von Humboldt, last above set forth, Dr. Jackson says—'Reflecting upon these phenomena, the idea flashed into my mind that *I had made the discovery I had for so long a time been in quest of*, a means, &c.' And he goes on to give formally and in detail the scientific deductions which he says were made at the time, and which then led him to the conclusion. If that statement be *true*, the *discovery* at that time, so far as private experiment and philosophical deduction could go, was as *full and complete* as it was on the morning of October 1, 1846, *after* Dr. Morton's successful operation on Eben Frost."

This report does not attempt to impeach, at all, the eight witnesses who *establish the fact*. They are Geo. Darracott, Esq., John H. Blake, Esq., Dr. Wm. F. Channing, Dr. S. A. Bemis, Dr. Geo. T. Dexter (of New York), Henry D. Fowle, Esq. (apothecary), D. J. Brown, Esq. (of the Census Bureau), Joseph Peabody, Esq. (of Salem, chemical student). To all of these gentlemen Dr. Jackson communicated this discovery as an ascertained fact, by experiments on his own body—as described by him in a communication to Baron Von Humboldt years before he communicated it to Morton. They are known to be all men of high standing and character.

This evidence was not before the Senate, being in the minority report of Hon. Edward Stanly, of North Carolina, and Hon. Alexander

Evans, of Maryland, the latter the most scientific member of the House; and the fact being well known that in all *special* committees the truth generally rests with the minority. The only thing before the Senate to controvert the monstrous report against Dr. Jackson, was a remonstrance signed by one hundred and forty-five physicians and surgeons of Boston and its vicinity, only three of them being out of its immediate vicinage. Two hundred were applied to, being such only as were members of the Massachusetts Medical Society. One hundred and forty-five of them, including many of the physicians and surgeons of the Massachusetts General Hospital, signed, and only three of those applied to expressed an opinion in favor of Morton's claims. They say in the remonstrance that—"the undersigned, residents of Boston and its vicinity, respectfully represent, that they have been familiar with the great discovery of the anæsthetic use of ether from its origin, and with the controversy following it. They now believe, and ever have believed, that Dr. C. T. Jackson is its sole and veritable discoverer, and that any merit on the part of W. T. G. Morton, the surgeons of the Massachusetts General Hospital, or of others, consists in taking his discovery after he had communicated it to many persons, in many places, known to some of us to be of unimpeached and unimpeachable character, and subjecting it to additional employment." (Signed by Dr. Luther V. Bell, of the Mc Lean Asylum, with 144 others.)

Mr. Walker asks, contemptuously, why Dr. Jackson did not go to Dr. Warren for his signature to the remonstrance. Dr. Warren has addressed a letter to Hon. Edward Stanly, under date of March 30, 1852, which gives an expression of his opinion; from which I quote the following remarks. "I have never learned any fact which has led me to doubt that Dr. C. T. Jackson suggested to Dr. Morton the employment of ether as an anæsthetic agent previous to any dental operation performed by him under its influence."

Such being the circumstance of this discussion, Mr. Editor, the impropriety of re-publishing the debate will be at once seen from the gross misstatements of facts bearing upon the question made by Senator Walker and others. Nearly every fact in the controversy is perverted and misstated, *while the case of Dr. Jackson was not presented in the Senate at all.* I will only allude to the principal and most pernicious of the misrepresentations of fact that were made during this attempt to get a "snap judgment" in the Senate.

First, Senator Walker, in behalf of Morton, says that—"a letter of Dr. Jackson procured upon the excitement of the moment a decision of the Academy of Arts and Sciences of France, awarding Dr. Jackson 2500 francs."

The fact is established by evidence that the decision was made *three years after* the letter was received, and after a thorough and full examination, not only of the claims of Jackson, Morton and Wells, but of one other American and ten Europeans, two of whom had better claims than either Morton or Wells.

Again, Mr. Walker says that—"still, so anxious was the Academy of Arts and Sciences to place in his [Morton's] hands an exalted recogni-

tion of his right, that they directed a certain portion of the fund to be paid in the shape of their largest gold medal." The inference which Mr. Walker evidently intended should be drawn from this, was that the Academy had had a *re-examination and revision* of their former decision which awarded this discovery to Dr. Jackson, with 2500 francs in money, and were instrumental in procuring for him the cross of the Legion of Honor, and at the same time awarded to a mechanical agent of his (Morton) "*un autre prix*" of the like sum for his services in bringing this discovery into practice, under the responsibility and direction of Dr. Jackson, deliberately assumed in the presence of Messrs. Barnes and McIntyre, two of Dr. Jackson's students; and that at a subsequent period they gave Morton their largest gold medal, which he (Walker) held in his hand. What are the facts? Why, members of the Academy are dissatisfied that they ever gave Morton anything. One of their most distinguished members writes with dissatisfaction and mortification of the Academy's having taken any notice of the claims of Morton for his comparatively insignificant services; while another correspondent accounts for it on the ground that certain interested parties had overstated his services in bringing the ether into practice. This inference assumed by Mr. Walker is contradicted by a letter under date of 21st of May, 1852, sent to Dr. Jackson by order of the Academy of Sciences, in reply to a letter addressed to that body by Dr. Jackson on the 30th of March, 1852, asking of the Academy, "if in according to Mr. Morton at the public session of March, 1850, one of the prizes concerning etherization, they have considered him as the inventor, or simply as *propagator* of the discovery"? M. Flourens, Perpetual Secretary of the Academy, replies as follows, "that he has *reason* to think that the terms in which the article that relates to you (M. Jackson) in the report of the commission on prizes is expressed, will leave you no doubt as to the true sense of the judgment of the Academy." The Secretary then recites the *procès verbal* of the Academy, making the original award of 1850, as expressing the unaltered opinion of the Academy, to wit: the maximum prize of 2500 francs from the Monthyon fund, for the greatest *discovery* in medicine and surgery, "for his (M. Jackson's) *observations and his experiments* on the anæsthetic effects produced by the inhalation of ether," and "un autre prix" (*another!* prize) "of 2500 francs to M. Morton for having *introduced* this method in surgical practice after the *directions* of M. Jackson."

It is obvious that the medal was obtained and artfully enlarged for the purpose of imposing upon Congress and the public. M. Elie De Beaumont writes, that in accordance with established usage, prizes can be taken out of the Academy either in money, or in a medal the value of which is $60. The medal in question was obtained of the Treasurer of the Academy, not by any action of that body, but by making use of a well-known custom in sending a portion of his 2500 francs to pay for a $60 medal; but the misrepresentation, not to say fraud, consists in representing this as a *second award* of the Academy. The medal, as Mr. Walker says, "was surrounded by a handsome gold rim," known to be an exact copy of the Clay medal; making the whole of the value of about $500,

and to look like one entire gold medal of an enormous size. Why was all this expense incurred by a man who gets up a subscription to pay his debts?

Mr. Walker further says, that the remonstrants referred to, "are Dr. Morton's rivals, men who had first given him notes, and then refused to pay them"; that "there are 300 medical men in Boston," and that "the remonstrants are scattered all over the State of Massachusetts."

There are but 200 physicians of the regular faculty in Boston, and of these 145 of the most eminent have signed, as before stated. These gentlemen will not feel much complimented by being charged with being the personal rivals of Morton and of "*owing him notes.*" This is all too ridiculous and contemptible for refutation.

I quote the words of M. De Beaumont, one of the most prominent members of the Academy of Sciences and Inspector-General of the Mines of France, and now member of the French Senate, relative to the award. In a letter to Dr. Jackson, dated "Paris, 17th of May, 1852," he says—"In *point of fact* the Academy of Sciences decreed to you one of the Monthyon prizes for the *discovery* of etherization." "And it has decreed a prize of 2500 francs to Mr. Morton for the application of this discovery to surgical operations."

"Now, all persons who receive a prize of the Academy of Sciences, can draw simply the sum which has been voted them, or they can draw a medal either of bronze, silver or gold. This medal bears a head of Minerva, and the superscription of the *Institute of France.* The cost of the medal is deducted from the sum paid. You have drawn simply the sum of 2500 francs, as is usual; but according to the information I have obtained from the Secretary, which was done to-day, Mr. Morton asked for a gold medal, the value of which is 300 francs [$60], and he has received in money about 2200 francs. In that Mr. Morton has but made use of a right which could not be contested, but the medal which he has obtained is the ordinary one of the Institute. It was not struck expressly for him. You have the right to ask for one exactly like it; only in that case you should receive but 2200 francs instead of 2500."

The correct state of opinion in France respecting this matter may be comprehended from the following extract from Figuier Decouvertes Moderne, Paris, 1851, tome i., page 219. "It is perfectly established for us, in spite of his assertions to the contrary, that Morton did not know anything about anæsthesia when, on the 30th of Sept. 1846, Dr. Jackson communicated to him, in a conversation, all that he knew about it."

Further, Dr. Joseph Weiger, Imperial Surgeon to the prisons and courts of justice, in a book published recently at Vienna, says—"In Vienna the operation of etherization is called 'Jacksonizing.' That is a new expression. People say Galvanizing, after the discovery of the physician Galvani; so should this discovery be called Jacksonizing, after the gigantic discovery of Jackson." In Sweden no one else but Dr. Jackson is considered as the discoverer, and the King has awarded Dr. J. his gold medal of merit.

The question of discovery is not *open;* the most eminent and impartial men of science have already settled it in favor of Dr. Jackson.

Why, then, was this foolish exhibition of a medal by Senator Walker, the larger proportion of which was a *fac simile* of the Clay medal, in the ratio of $500 to $60, and that medal might have answered Mr. Walker's purpose quite as well, if it had not unfortunately been stolen somewhere between Washington and New York. M. Velpeau, the surgeon, who is represented in the debate as "sneering" at Dr. Jackson, has repeatedly and fully ascribed in his public lectures to the Academy the discovery of etherization to Dr. Jackson (see the Comtes Rendus of the Academy).

Mr. Walker further says, "*We* have the award of a casket and $1000 by the Trustees of the Massachusetts Hospital"; "We have two reports of the Hospital of Massachusetts, &c."

Every one of the Trustees of the Hospital, in letters addressed to Mr. Stanly, except N. I. Bowditch, deny that the Trustees of the Hospital ever had any judicial investigation of the subject; that Dr. Jackson never appeared before them either in person or by counsel; that Mr. N. I. Bowditch is alone responsible for the reports, and that the Board never did act as umpires between Drs. Jackson and Morton, and never intended to do so. The signers of the subscription for the "casket," Hon. Josiah Quincy, Jr., Hon. Abbott Lawrence, Hon. Wm. Appleton, Hon. Wm. H. Prescott, and others, all deny that they gave their $10 for any such purpose as giving a testimonial in honor of the ether discovery. Hon. Josiah Quincy, Jr. says—"Understanding that Dr. Morton was in very straitened circumstances, I headed a subscription in his behalf, in order to pay off his debts, and relieve him from an execution, with which I understood he was threatened." Mr. Lawrence says the same thing, and adds—"I gave most cheerfully, because *Mr. Bowditch* desired me to do so." He has further said, that all the gentlemen who signed gave under the same idea.

Mr. Walker inquires how Dr. Jackson obtained any interest, and answers by saying—"It was through the mistake of the lawyer employed in obtaining a patent." This *lawyer* (patent solicitor), as shown by a transfer at the patent office, was interested in the patent to the extent of one fourth before he acted as the pseudo-legal adviser of Dr. Jackson. Is it likely that he would make a mistake? He knew too well that the patent was of no use without the name of Dr. Jackson to it, and that when Dr. Jackson denounced it as fraud, into which he was inveigled by a misrepresentation of his rights, "that he could not otherwise conserve his discovery," the patent became invalid, and of no use to the man who is now attempting by such very *queer* means to sell it to Congress. In the first place, the discovery is not patentable; and in the second place, the discoverer of this new fact, could not by any manner of legal instrument deed it away; as in the nature of things a man cannot transfer a *discovery*, though he may convey to another the right to use it. Mr. Morton early in October, 1846, offered Dr. Jackson $500 for the right to use this discovery in his dental practice, before he found it desirable to get a patent-solicitor to get Dr. Jackson's name to a patent. This is admitted by all Morton's advocates, and all the statements on Morton's behalf; and is admitted page 53 of the majority report, page 33 Hospital

report, page 10 Letheon, and page 59 Morton's memoir to the French Academy, though not one cent has yet been paid for it, *nor has Dr. Jackson ever received anything* from him, nor would he demand it or accept it. He, however, has lived in the hope that the spontaneous gratitude of his countrymen would reward him and refund the losses of time and money he has sustained by the cupidity of others.

Boston, November, 1852. H. A. HILDRETH.

CHLOROFORM AND CHLORIC ETHER.

Remarks of Dr. J. C. Warren to the Medical Class and other Gentlemen on a Death by Chloroform, accidentally administered in the Massachusetts General Hospital Nov. 6th, 1852.

THE occurrence in this Hospital a few days since of two unfortunate cases, one of which proved fatal, renders it desirable that an explanation should be made of their course and cause. I shall attempt this explanation the more readily, as it will afford a great practical lesson, which you may never again have an opportunity of witnessing.

This hospital was founded about thirty years since. The most distinguished men of our community, the Lowells, Bowditches, Quincys, Sullivans, Perkinses, Lawrences, Appletons, and others, were among those who devoted their time, talents and property to the erection of the Institution, and who have continued to support it by their paternal care. These names are a pledge to the community that it has been conducted with all the wisdom human ingenuity could furnish.

Among the regulations which they have thought judicious, is one for an annual election of the medical officers. This regulation does not involve the necessity of removal of all these officers; they are changed only when it is thought necessary, or expedient; and this necessity does exist frequently in regard to the house-surgeons or pupils, and the apothecary. When it takes place in the latter office, a considerable time is required for the new incumbent to acquaint himself with the customs and practices of the Institution; and it is obvious that such a change, involving an immense number of details, cannot occur without some oversights and mistakes. No mistakes, however, of any importance have come to our knowledge previous to those connected with the late accidents.

A number of operations were to be performed, and the patients to be etherized. For this etherization, Chloric or Sulphuric Ether is usually employed. Chloroform, the popular favorite, is never used in this institution, except as an external application; but on this occasion it was introduced from the fact, that it had been poured into a bottle labelled with the title "Concentrated Chloric Ether." This bottle, then, marked as concentrated chloric ether, was placed on the table, and employed for these operations without suspicion on our part, that it was not the article designated by the label on its surface. The error escaped the observation of those who administered it, from the fact that there is a resemblance in the sensible qualities of the two articles. So that three persons were etherized with chloroform instead of chloric ether.

Many of you will immediately inquire, why we object to the use of

a substance employed by men of science, and men without science, all over the world, as the article best adapted to produce anæsthesia, or insensibility to pain. The ground of our objection to the use of chloroform is its tendency to produce dangerous consequences in a certain number of cases. The published fatal cases of chloroform now known cannot be less than thirty; and it is well understood, that many have never met the public eye. The appearance of fatal cases in 1848 led me to publish my remarks on the "Effects of Chloroform and Strong Chloric Ether as Narcotic Agents." Besides the Boston edition of these remarks, editions were published in the Medical Journals of Philadelphia and of London, and by these various publications the dangers of chloroform were made sufficiently known. Most, if not all my colleague surgeons in the Hospital, influenced by the same considerations which had guided me, concurred in disapproving its use. In the mean time, sulphuric ether in the enthusiasm for chloroform had been almost forgotten, and the question arose, whether we should return to it. The objections to its use were its pungent odor, its stimulating power, its extensive diffusibility, the subsequent dead smell, and finally its great disposition to produce nausea and headache in those exposed to its influence.

At that period many physicians were engaged in searching out new anæsthetics, and some of us thought it might be useful to unite in this investigation. An article used in medicine for a number of years back, and also as a favorite drink, the chloric ether of commerce, attracted our attention. We made trial of it, and found it possessed no anæsthetic properties. Having witnessed in the distillation of chloride of lime and alcohol the production of a liquid of different degrees of strength, part of it being much more powerful than the rest, we consulted an able chemist, and found, that by a re-distillation of part of it with the addition of two parts of alcohol, there was formed a beautiful chloric ether, which we hoped might be safe, effectual and agreeable. We therefore made a cautious trial of its effects on a patient in the Hospital, whose irritability was such as to prevent an investigation of his disease; the ether having been administered, the examination was immediately effected with perfect ease and success. Proceeding with great care, we gradually extended its use to all cases where chloroform had been employed, and found it "agreeable, safe, and effectual." Soon after, the subject having been introduced at the meeting of the American Medical Association in Baltimore, I was called on to say what I knew of its effects; and the remarks I then made being favorably received, I published an account of it in connection with the work already alluded to, recommending it as a substitute for chloroform.

Objections were taken to this substance on account of its alliance to chloroform, and articles were written to show, that it could not be used with safety; but I did not think myself justified in abandoning its use. Perhaps it will be thought I ought to have replied to these objections; but having already employed much time and labor in the investigations relating to this ether, and having fairly placed it in the hands of the public, I could not consider it a duty to go into a controversy for its support. Since then, I have given it in a great number of cases, and no fatal occurrence has ever followed the administrations.

In the course of the last summer I had some communications on this subject with Dr. Bache, the distinguished editor of the U. S. Dispensatory, and also with Dr. Hayes, of this city; in consequence of which, the following letters were written at that time, viz., in the month of June, 1852. The statements of Dr. Hayes appeared to me very important, but I was still unwilling, for reasons already alluded to, to bring them before the public. Although there is no other connection of these two articles in the cases we are about to speak of than that arising from the similarity of their physical qualities, there seems to be a propriety in making the letters public at this time:

LETTER FROM DR. WARREN TO DR. BACHE.

Boston, June 8, 1852.

My Dear Sir,—About a year since, viz., in May, 1851, I had the pleasure of conferring with you on the properties of strong chloric ether as an anæsthetic agent, with a view to a description of the same in a new edition of your valuable Dispensatory. I promised that I would, on my return home, write to you on the subject, and did accordingly write, giving an account of the actual state of the ether practice in our Hospital. One fact, however, of importance had not then come to my knowledge.

The distilled chloric ether originally employed by my advice, had been, from some reason, changed for a mixture of alcohol and chloroform, or a tincture of chloroform. As soon as I discovered this fact, I applied to a chemist, who promised to prepare the distilled article. Circumstances prevented his doing so, and thereon I conferred with my friend, Dr. Hayes, a distinguished philosophical chemist, who readily agreed to give the necessary instructions to Mr. Atwood, a practical chemist connected with the firm of Philbrick, Carpenter & Co., Washington street, Boston, for the right preparation of strong chloric ether, or, as Dr. Hayes prefers to call it, 'Compound Chloric Ether.'

This preparation I began to use in the last autumn, and have, with Dr. J. M. Warren and other gentlemen, continued to employ it. Dr. Hayes's views on this subject you will be able to learn by the note following, which he has had the goodness to address to me.

In regard to the case of death to which you allude, supposed to be from tincture of chloroform, there seems to be some reason for doubt. But as this was not the preparation which I am in the habit of using, I should not advise it, and I have reason to believe it would not have been employed in this case, had its difference from distilled chloric ether been appreciated.

In my remarks on chloroform, published in 1849, after describing the properties and effects of strong chloric ether, I have said—'Should any one, preferring chloroform to the ethers, feel disposed to employ this article in a diluted state, he might very properly add a certain portion of alcohol.' This preparation I have never used to any extent, and am now satisfied that it has different properties from those I supposed it to possess.

NOTE FROM DR. HAYES TO DR. WARREN.

ETHER, CHLOROFORM, AND TINCTURE OF CHLOROFORM.

Chloric Ether.—This substance is the product arising from the action of hypochlorites of the alkalis, alkaline earths, on a large excess of alcohol, much diluted with water. It is obtained by distillation, and when carefully prepared contains chloroform, chlorinated ether, and alcohol. In its formation, a large quantity of acetic acid is produced, and unites with chlorine and the base of the hypochlorite used in producing it.

It is a permanent compound, possessing the grateful odor and sweet taste of chloroform; when evaporated from the hand or clean linen, it leaves no odor adhering to the surface. In this state it is efficient and convenient for use, as an anæsthetic agent. It is indefinite in composition, but when decomposed by mixture with two bulks of water, it should deposit about one third of its original bulk of heavy oily fluid.

The extended use of this substance by some of the surgeons of the Massachusetts General Hospital, has led to the attempt to substitute for it, the tincture of chloroform. It will be seen that these are not like bodies, and as it is more difficult to prepare chloric ether than chloroform, the manufacture of the former will doubtless remain in the hands of the skilful pharmaceutists.

Chloroform.—This substance, as a secondary product, is found after many reactions, in which chlorine and hydrocarbons are present. When obtained from hypochlorites and alcohol, the proportion of the latter substance is very small, relatively to that of the hypochlorite used. After careful purification it is a definite compound of well-known physical characters. There is, however, an important chemical character recently observed, which should form a part of its history—*it is decomposed by solar light.* In the early stages of its changes, the odor remains fragrant for some time, but is succeeded by a suffocating and corrosive vapor, arising from the action of hydrochloric (muriatic) acid on hydrocarbons present. If the remaining chloroform is carefully washed and purified, and again exposed, the same changes succeed; conclusively proving that the property is inherent.

The risk attending the use of compounds having the same odor, but really foreign in composition arising from the use of alcohol, which contains fusel oil in the manufacture of chloroform, has been already pointed out in the Medical Journals.* There is, however, a preparation sold under the name of Tincture of Chloroform, which is objectionable, and as it has been substituted for chloric ether, has been examined.

When chloroform is added to alcohol of 85 per cent., it dissolves until about double the volume of the alcohol has been mixed. After subsidence, a singular change has taken place; the water, fusel oil and some alcohol unite to form a layer on the surface of the dense alcoholic solution of chloroform. This may be removed, but the solution remains too strong for use. Any alcohol of the shops added, introduces water, hastening the change which chloroform undergoes. When anhydrous alcohol is used, unless distillation has been a resort, the tincture is subject to the same change from neutral to acid state, as chloroform exhibits. After such change hydrochloric acid may be found in it uncombined, unfitting it for any use.

Theoretically and from observation, the compound chloric ether seems to be the most permanent and convenient form in which the power of chloroform can be exhibited, and as such, should take the place of chloroform, in medical and surgical practice.

During the past summer, two or three deaths are said to have occurred from the use of the tincture of chloroform, under the name of chloric ether, an article different from the true chloric ether, as pointed out in the preceding papers. It consists of a mixture of alcohol and chloroform, in the proportion of two to one, without distillation. This mixture was adopted no doubt as more economical than the distilled compound chloric ether—a fact which accounts for its introduction and substitution for the other, but I have never used it, intentionally, except for the purpose of experiment, and by the aid of Dr. Hayes I have obtained a pure distilled compound chloric ether, as mentioned above. From this article, so far as I know, no fatal cases have ever occurred.†

* Vide Boston Medical and Surgical Journal, Jan. 21st, 1852.

† The following correct method of preparing this chloric ether has been kindly furnished me by Mr. Atwood.

"In my process for the production of chloric ether, the alcohol is perfectly freed from fusel oil. A larger proportion of alcohol and water are also employed than in the manufacture of chloroform. The following are the proportions I use, viz.:—Chloride (hypochlorite) of lime, 10 lbs.; Water, 8 gallons; Pure alcohol, 1 gallon; Carbonate of soda (crystallized), half pound. Break down the chloride of lime in the water until the excess of hydrate of lime is in a uniform pulpy mass, and the chloride is perfectly dissolved. Place the mass in a still capable of containing twice the quantity, and introduce the alcohol. Mix perfectly and apply a moderate fire under the still until distillation commences. Continue the distillation as long as a portion of the distillate will

This ether is used in this city, in Salem, in New York, the South and West, and is considered a beautiful and valuable article. Families who have employed it here, have sent from Europe to obtain it. Chloroform, as already stated, notwithstanding the mortality which has followed its use, is more extensively employed all over the world than any other article, and this happens because it is more speedy in its effects than any thing else. Sulphuric ether, although there have been scarcely any, or very few authentic accounts of its fatal effects,* is not known to be employed to any great extent in other places than Boston.

I shall now proceed to give an account of three cases, in which chloroform was accidentally administered in this Hospital on last Saturday. In my account, I shall endeavor to avoid the use of names, as more delicate and proper; but I would say that all who aided me on this occasion, performed their duties with activity and with credit.

Three cases presented themselves for operation. The first was on a contracted hand. The patient was etherized with the supposed chloric ether. In two or three minutes, anæsthesia being produced, the operation was performed under the continued administration of the same article during from five to ten minutes. The patient escaped without any other inconvenience than a slight soreness of the throat—an effect of the inhalation of chloroform which I have myself experienced.

The second case was of a tumor on the right side of the face in the region of the parotid gland, supposed to lie in the substance of this gland. The attendants being arranged so as to give every assistance to the patient and the operator, the anæsthetical liquid was applied as usual with a sponge, and with the freedom employed in the use of ether, but not proper where chloroform is known to be used. As soon as the application was made, the patient began to struggle and throw his limbs about in so violent a manner that we were compelled for the moment to resign him into the hands of the assistants. But being soon exhausted by the excessive motion, he necessarily inhaled more freely, thus filling his lungs with the vapor of chloroform, and in three or four minutes rendering him insensible. The operation was begun, the parotid gland laid bare, the tumor found to lie behind it, the parotid gland itself incised, and a round regular tumor enclosed in a fibrous sac was brought into view. At this moment those appointed to watch the patient gave a signal that the pulse was failing and respiration scarcely perceptible. Immediately cold water was dashed on his face, and this not reviving him, motions of the chest (in imitation of respiration) by moving the ribs up and down, blowing of air into his face, and afterwards into one nostril, stopping the other (pressing back the larynx so as to prevent the air

deposit chloroform on being mixed with its bulk of water, then change the receiver and collect one gallon of alcoholic liquid.

Add water to the first portion of the distillate as long as chloroform is precipitated. Separate the light liquid from the chloroform, and wash the latter in twice its bulk of water containing the carbonate of soda. Separate the chloroform from the carbonate of soda and weigh it.

Mix the chloroform, washings, and alcoholic liquid in a still placed in a water-bath. After twenty-four hours' repose, distil three times the weight of the chloroform, and mix perfectly. Preserve in well-stopped bottles.

The 'chloric ether' must not redden litmus paper, or give rise to a precipitate when mixed with a solution of nitrate of silver."

* Vide British and Foreign Medico-Chirurgical Review, Jan. 1852.

from going into the stomach), frictions of the limbs, clearing the mouth of saliva and froth, and removing the mucus from the throat by the finger, were resorted to. At this period I called for ammonia. During the past year I have been in the habit, when a patient was to be etherized, to direct a bottle of ammoniated alcohol to be placed on the table. This substance is a powerful stimulant, and has been employed from an ancient period to revive persons affected with many of the forms of suffocation, or asphyxia, and also for fainting. Its object is not to oxygenate the blood, which it cannot do, but to give a spur to the nervous system, and put in motion the dormant vital energy, for which purpose it is superior to any other stimulant. This is not the strongest preparation, but there was brought by mistake, and without my being aware of the fact, a bottle of aqua ammonia, which is three times stronger than that above named. It has the same color, and also the same odor, though the latter in a greater degree, with the ammoniated alcohol. The difference, however, would not be detected in a case of urgency, where a few seconds lost might be fatal to the patient. This article then was applied on a sponge to the nostrils, and not producing any effect, a small portion was insinuated into the mouth. At this moment some faint appearance of respiration was exhibited, and by the continued efforts of artificial respiration and friction, the pulse returned, the patient began to breathe freely, and in from five to ten minutes more he seemed quite out of danger. The operation was then concluded, and the patient carried to his bed.

After lying a few hours he recovered his usual state, at least so far as to speak and drink without difficulty; he was, however, much troubled with a secretion of mucus from the lungs, and a cough necessary to extricate it. He had also soreness of the throat in swallowing. The latter symptom has at this time disappeared, but the cough and expectoration continue. The patient wishes to leave the Hospital this afternoon, but I shall previously take occasion to call him into your presence.

The third case is the most important. It was that of a young man, about twenty years old, a native of Ireland, who had had his arm entangled in the machinery of a bark-mill about five days before. The muscles and other organs were torn from the fore-part of the arm, and some loss of blood took place. On his entrance to the Hospital the hand was found cold and without sensation, showing that the nerves had been destroyed, and that his arm could not be restored. Amputation was proposed to him, but he rejected it, and notwithstanding the danger of mortification and lockjaw, and the ultimate uselessness of the limb, were pointed out to him, he insisted he would die with his arm on.

On Saturday, partial mortification having taken place in the mean time, rendering the arm excessively painful and fœtid, and being convinced that he could never recover the use of it, even if he lived, he agreed to have it amputated. The surgeons and assistants took their places around him, as in the last case, while I myself watched his pulse. Etherization was carefully made; immediately on the application of the anæsthetic fluid he became perfectly quiet; the operation proceeded, and was accomplished in about two minutes. Just as it was finished, I perceived his pulse was rapidly failing. Word was given to suspend the dressing, and dash water on his face, which was immediately done. Notwithstanding this the

respiration and pulse went on diminishing, and soon ceased. He was to all appearance entirely dead. Artificial respiration was directly produced by moving the ribs; the limbs were rubbed, ammonia was momentarily applied to the nostrils and mouth, and when these things failed, ammonia was introduced into the mouth, as in the other case. Soon after this, to our great joy a slight inspiration followed, and the efforts being continued, his respiration improved, though he breathed with difficulty, owing to the quantity of mucus in the lungs. By great efforts on the part of the gentlemen standing around, in lifting and turning him on his side, so as to drain out the mucus from his lungs, and by frequently sponging the back part of his mouth, he was from time to time relieved. At last, passing an empty spoon into his mouth, and pouring some brandy and water from another into it, he was made to swallow fully. A stimulating injection into the bowels was also administered.

After aiding in clearing his lungs for some length of time, it was thought he might be removed to his bed; there a little brandy and water was given occasionally, which he swallowed readily. He also spoke and answered all questions proposed to him until the last moment, showing that the organ of voice was not injured. When asked if he suffered, he said "yes," and placed his hand on the region of the heart. Mucus continued to fill his throat. There was no obstruction in the opening of the larynx, for mucus issued from it in a copious stream, showing that his whole lungs were affected. Having remained with him until the pulse had become pretty good, and the respiration apparently better, we adjourned, to meet again in an hour and a half; placing at the same time the house surgeon at his side, with instructions to keep his throat clear of mucus, and support him by stimulants, with the strongest injunctions not to leave him till our return.

Shortly before the time fixed for the return of the surgeons, which was half past three o'clock, the house surgeon perceiving his pulse to suddenly fail, and that his breathing was more hurried, uncovered the stump to see if it was bleeding, and found some effusion of venous blood, probably produced by the liquefaction of blood from the chloroform poison. He then cleared the mouth of mucus, which he had hardly completed when the patient breathed his last without any effort or convulsion. Soon after, an opening was made in the trachea and air blown into the lungs for the purpose of inflation, but without effect. A proposal had been made to do this during life, but it was objected to, because air had already been thrown into the lungs through the nostril, because there was no obstruction in the larynx, because blood might escape through the aperture into the trachea and combine with congestion and mucus in the lungs to increase the difficulty.

On the following morning an examination of the body was proposed, but his friends arriving, objected, and although we urged the importance of ascertaining the immediate cause of death, they continued to object decidedly.

Remarks.—Immediately after the occurrence of alarming symptoms in this case, it was discovered that the substance which had been used was not chloric ether, but chloroform; and not till then did we understand the extraordinary phenomena which presented themselves in this and the preceding case. This patient died with the usual phenomena of chloroform poison.

If we consult the records of fatal cases of chloroform, published by me

in 1849, we shall perceive that of fifteen cases there mentioned, the principal part took place in a very sudden manner, some of them occurring a minute or two after the application, and some of them in a period of from ten minutes to fifty hours. In the latter cases the lungs were remarkably congested or filled with blood, owing to the poison applied to the air-cells of the lungs, or circulating with the sanguineous fluid, as in asphyxia. From various causes asphyxia is of frequent occurrence; the phenomena are the same with those presented in these cases, and the remedies are the same—hence the great importance of being well acquainted with the treatment adopted in all such casualties.

The first class seem to have perished almost as if they had been struck by lightning, the powers of the nervous system appearing to be at once annihilated. In the second class the lungs exhibited a most remarkable state of congestion. In the death at ten minutes after the application, the lungs were "a good deal congested; and discharged, when cut, a large quantity of bloody serum." In the death in three quarters of an hour no examination was made, but "the respiration was infrequent and sighing," showing that the function of the lungs was interrupted. In the third case, "the lungs were filled with blood and softened; bloody serum in pleura." In some of the cases the heart was found disordered, in others the brain, but in the whole number, I believe without exception, the lungs were charged with blood, or congested—the common, decided effect of chloroform.

The revival from anæsthetic symptoms and prolongation of life for ten minutes, three quarters of an hour, and fifty hours, bring these cases into the same category with ours. The vital principle after appearing to be extinguished lights up, and gives the hope of recovery, but the blood continuing to accumulate in the lungs from the effect of the poison, and the weakness of the patient, its oxygenation is prevented, and from want of the animating principle life is suffocated and extinguished.

Is the fatal termination of the third case to be attributed to any cause other than that which exists in the preceding cases? There is no reason to believe this to be the fact. The sinking after revival might lead to a suspicion that some other than the usual cause—congestion of the lungs—existed; and some one has suggested that it might have arisen from ammonia having entered the lungs. How could it be the cause of death in this case? By being introduced into the lungs and irritating and burning these organs? This was impossible in the given circumstances; the patient neither swallowed nor breathed after the ammonia had been employed until the whole had been washed out of the mouth by the abundant mucus. It may have been thought that the ammonia irritated the opening into the larynx and swelled it so that no air could pass through. The air did pass through freely till he died, and so did the mucus, a less volatile fluid than air. Further, when ammonia is introduced into the mouth, its entrance into the larynx is repelled by contraction of the laryngeal muscles, so that it cannot enter. Moreover, it was in excessively small quantity. A saturated solution of nitrate of silver, it may be remarked, is frequently introduced into the mouth and even larynx for curative purposes. Most of the ammonia immediately ran out, and the rest was thrown off by the mucus of the lungs and throat. Had any disorganizing effect been produced, how could the patient have swallowed repeated draughts of brandy up to the period of his death? Finally, if the ammonia destroyed the last patient, how did the second escape the action of the same cause? He used the same quantity, so far as can be judged, and was able to swallow through

the day and ever since, and to take even solid food. True he was stronger than the other, but this difference of strength would have made no difference in the chemical, or even vital action, more favorable to the one than to the other; yet the one is well—the other is dead. But I will insist no further on this point, and perhaps have already said more than was required.

We believe that the temporary resuscitation of one of these individuals, and the permanent restoration of the other from apparent death, must be considered as a triumph honorable to Medical Science and to this Institution.

THE BOSTON MEDICAL AND SURGICAL JOURNAL.

BOSTON, NOVEMBER 24, 1852.

To Readers and Correspondents.—As will be noticed by the reader, much space has been devoted in the present number of the Journal to Dr. Warren's explanation of a recent unfortunate occurrence at the Massachusetts General Hospital. The peculiar circumstances connected with it seemed to require that this large space should be allowed for an explanatory statement, although it had previously appeared in the columns of the American Traveller, and although, by inserting it at the late period when it was received, we have been obliged to postpone the continuation of the interesting papers of Drs. Channing and Coale, besides the insertion of a number of others which have been awaiting their turn. A statement by Mr. Hildreth is also contained in the Journal to-day, which was considered by the friends of Dr. Jackson to be called for, in reply to some of the assertions in the debate of the United States Senate, recently presented to our readers in a supplement. As in the case of that supplement, we are not answerable for anything contained in the present statement.—The following papers are on file, in addition to those already acknowledged:—Media Sacra; Thermometrical Observations in Missouri; Medical Application of Electricity; and Use of the Stethoscope in Midwifery.

Diseases of the Mouth—Apparatus for Cleaning the Teeth.—A compilation of aphorisms on the prevention and treatment of diseases of the mouth, prepared agreeably to a resolution of the American Society of Dental Surgeons, has been some days on our table. Dr. L. S. Parmly, a distinguished dentist, has contributed largely to this collection of wise directions. He is also the inventor of some singularly shapen instruments of white clay, which are used for polishing the teeth, and giving to them a bright, healthful appearance. Our food is so fine, in these days of civilization, that the teeth meet with little or no resistance in crushing morsels, and hence a want of friction, which in lower animals is most perfect, in gnawing and tearing their food, and which keeps their manducatory apparatus in excellent condition. Dr. Parmly is an experienced man, and whatever he says in respect to the beneficial result of his clay polishers, is taken for truth, although the instruments are a novelty, and at first sight strike the eye as being better in theory than in practice.

Deaths in Boston—for the week ending Saturday noon, Nov. 13th, 72—Males, 29—females, 43. Accidental, 2—inflammation of bowels, 1—inflammation of brain, 4—bronchitis, 2—consumption, 14—convulsions, 4—croup, 4—diarrhœa, 1—dropsy, 4—dropsy in the head, 2—infantile diseases, 7—puerperal disease, 1—scarlet fever, 9—gangrene, 1—disease of heart, 2—intemperance, 1—inflammation of lungs, 4—disease of liver, 1—marasmus, 1—old age, 1—palsy, 1—pleurisy, 2—teething, 1—unknown, 1—worms, 1.

Under 5 years, 23—between 5 and 20 years, 9—between 20 and 40 years, 16—between 40 and 60 years, 11—over 60 years, 8. Americans, 32; foreigners and children of foreigners, 40. The above includes 3 deaths at the City Institutions.

Death and Post-mortem Examination of the Hon. Daniel Webster.—To the Editor, &c.—Sir, The medical public have been waiting with great patience for an accurate report, in your valuable Journal, of the disease which terminated the life of this eminent statesman, and the results of the post-mortem examination said to have been made. Why does it not appear? A paragraph has gone the round of the *newspapers*, purporting to be authorized by his medical advisers, in which, if my recollection serves me, language something like the following is used:—"Mr. Webster died of a disease of the liver. There was also a morbid state of the blood, consequent upon this disease, and dropsy of the bowels." Now, in my humble opinion, this *report* is vague, meagre, unsatisfactory and unscientific. Something more is required, and we have a right to demand it of those medical gentlemen who were fully cognizant, by their position, of all the facts. There is an air of mystery over the whole circumstances, which we wish should be cleared away. Let this be done. Let us have a report which shall be full and minute in all particulars. Let this be *first* published in the Boston Medical and Surgical Journal, where it of right belongs, and we will render honor to whom honor is due. Cullen.

[The above is one of many inquiries which have been made to us respecting the post-mortem examination of Mr. Webster. In reply, we would state that we have made all proper efforts to procure the intelligence sought for, but have not succeeded. It is said, though we have not been able to learn anything definitely, that the whole matter will be communicated to a Journal at the South. No reason has been assigned for this singular proceeding. Should it be sent off to a distance, instead of being published in Boston, which seems to be the appropriate place, for the sake of our readers we shall probably inform them where they may find and read it.]

Ammonia Used for the Poison of Venomous Animals.—A physician of this city who has resided sometime in the Southern States, informs, that it was customary to administer ammonia for the cure of the bite of a rattlesnake and other venomous reptiles. He was knowing to a case of this kind in a servant who was bitten by a rattlesnake. His master being provided, gave him a teaspoonful of caustic ammonia; the whole of it was swallowed, producing a soreness of throat which lasted three or four days, and then went off. No subsequent inconvenience was produced, and the patient experienced no ill effect from the bite.

Medical Miscellany.—Dr. George C. Shattuck, of Boston, distinguished for his liberality and goodness of heart, has presented Dartmouth College $7000 towards the erection of an observatory, on the condition that the trustees of the college will raise the further sum of $8000 for the purchase of instruments.—Smallpox and hooping cough are fatally in the ascendant in Demarara.—Drs. Barth and Oberweg, the great travellers, at the last accounts were making extraordinary explorations in Africa.—Public health at the Isthmus of Panama, is improving, there being but few cases of fever, and no cholera.—The climate of New Zealand is represented to be the finest in the world.—Thomas Lippencott, a young man now lying at the Commercial Hospital, Cincinnati, has injured one of his feet to such an extent, by the practice of wearing tight boots, that it is necessary to have it amputated.